depending on the height of the barometer. M. Vallot adopts the same form of expression, but unfortunately does not give the means of comparing his numbers with those found in the earlier investigation. Moreover, the methods employed are not quite the same, and a slight difficulty arises from the selection of a silver salt whose maximum effect is not exhibited at the same part of the spectrum as in the case of Bunsen and Roscoe's inquiry. Making allowance for a slight discrepancy in this respect, we gather that while Bunsen and Roscoe claimed that 59 per cent. of the solar light was lost by transmission through the atmosphere, M. Vallot finds that only 40 per cent. is so lost. Treating the sensitised paper in such a way as to make the maximum sensibility coincide with the optical maximum, M. Vallot finds the coefficient of atmospheric transmission to be 0·826, corresponding with the mean of the values found by Bouguer, Leiden and Trépied. While the agreement quoted is quite satisfactory, we miss any reference to the work of Captain Abney on the Faulhorn (2683 m.), of Dr. Muller at the Etna Observatory (2944 m.), or of Prof. Langley on Mount Whitney (3513 m.). These latter authorities have all found a rather larger transmission coefficient than others who have worked nearer the sea level, and the greater altitude of the Mont Blanc Observatory might have afforded some explanation. Doubtless if the object be to inquire into the effect of the variation of a particular factor, there is an apparent advantage in conducting the observations in such a way that that factor is most materially affected, but the advantages can be considerably discounted by the extra difficulties introduced by the remoteness of the situation and other causes, and though M. Vallot and his assistants have striven manfully with the difficulties, greater weight would attach to his results if the observations had been more frequently repeated, though at a lesser height and with a smaller diminution of the barometric pressure.

The means employed for discussing the relative velocity of water in streams and torrents is discolouration of the water by means of a powder (fluorescin), which on dissolving rapidly gives rise to a green tint, and then observing the time at which the discoloured water arrives at a distant station. M. Vallot very properly insists upon the necessity of a sufficient expenditure of the powder, which can be determined experimentally according to conditions varying with the quantity of outflow that passes a given point, the colour of the water, &c. Observations made in this way do not permit of very great accuracy owing to the tendency of the coloured water to spread itself according to the character of the channel over which it passes, but when it is a question of determining the velocity in a subterranean channel, under glaciers, it is difficult to see what better means could be devised. M. Vallot has confined his attention mainly to the effect of the slope of the river bed on the rate of flow, and concludes that this is most rapid when the incline is about three in a hundred. As the slope increased, contrary to his expectation, the rate as measured horizontally, diminished. From this he concludes that the onward rush of water along steep ravines is more apparent than real. When the incline is very steep the effect is to hollow out the bottom, giving rise to great irregularities in depth which produce eddies and tend to stop the hurrying progress. It is therefore the mean velocity with which M. Vallot is concerned, and the distances measured vary from 1000 to 4000 m. The maximum velocity observed in the unconfined stream is 2·25 m. per second, and for a subterranean current of the same slope the progress is about one half of this quantity. The same ratio obtains between open streams and sub-glacier currents, whether the horizontal or vertical velocity be measured.

But the particular investigation to which M. Vallot has devoted the greatest attention is the movement of the glacier, Mer de Glace, in its various parts. Here the author enters into a very difficult inquiry, and it would be surprising if some of his results did not differ from generally received opinions. As a contribution to a more complete investigation the study made by M. Vallot is no doubt valuable, and much of his work, such as the difficult triangulation of the Mer de Glace, will be appreciated by later observers. But a complete theory of glacier movement is not to be derived by a study, however minute, of a single glacier in the space of a few years. An international commission under M. Forel has been at work for a considerable period and is probably still collecting data which show that the problem possesses many perplexing variations, not the least difficult being the evidence of periodicity in glacier movement connected with some obscure law that appears to affect the general climate of the earth. During the eight years that M. Vallot has been at work on this subject, he claims to have established the following prominent facts, which undoubtedly have reference to the particular phase of the motion which obtained in the period to which his observations have reference. We regret that it is impossible to enter with minuteness into the character of the evidence by which his conclusions are supported, but they may be briefly summarised thus: (1) That the progress of the glacier throughout the year is constant, the summer does not hasten nor does the winter witness any abatement of the uniform progress. Changes of incline of the bed on which the glacier moves will always explain any observed variation of velocity. (2) That the uniformity of the velocity in all seasons is opposed to any theory of regelation, or, indeed, to any explanation in which changes of temperature play a part. (3) That the movement of the glacier does not partake of the character of a viscous fluid, the whole moving as one piece. M. Vallot thus sums up the result of his long and arduous labours, which have been pursued under great difficulties with considerable skill and over a large area. We can only hope that the experience he has gained will be still longer employed in this species of investigation. "La conclusion de ce travail est que *la progression des glaciers est causée par le glissement de la masse, sous l'action de la pente du lit, aidée par la poussée des parties postérieures*. La pesanteur seule parait être en jeu, à l'exclusion de toute action calorifique."

GEOLOGY AND METEOROLOGY.

THE subject of climatic changes has always been of absorbing interest to geologists, and they have been perhaps more puzzled to account for the occurrence of plants of temperate or even subtropical character in Arctic regions than for the occurrence of wide-spread Arctic conditions in temperate regions. To explain these changes in the northern hemisphere, the alterations in the distribution of land and water and consequent influence on the Gulf Stream, the modification of the internal heat of the earth, changes in the position of the earth's axis, variations in the amount of heat given off by the sun, the eccentricity of the earth's orbit, and even fluctuations in the amount of carbon dioxide in the atmosphere, have individually or collectively been invoked. The influence of ocean currents, as modified either by the elevation of a tract of islands to form a continental area or by the total or partial submergence of a continent, has naturally been regarded as of very great importance. Moreover, the effect which such changes would have on winds has not been neglected, although their local influence has not been fully realized.

The apparently wide extent of tropical and subtropical climates during past epochs, with evidence of progressive diminution in temperature in later Tertiary times, has been held to be due to astronomical rather

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than geographical causes. According to M. Eug. Dubois[1] one obscure enigma is that relating to the glacial episode which has been recognised in parts of India, Australia and South Africa in Permo-Carboniferous times. In those subtropical regions the débris from snow-clad mountains had been able to reach sea-level and be commingled with organic remains of almost tropical character. Other evidence, however, tends to show that there was no general lowering of temperature in this ancient epoch, but that there must locally have been mountains of considerable altitude, and that meteorological conditions were favourable to the development of huge glaciers. So also in the case of the far earlier pre-Cambrian period, during which it is believed that glaciation occurred. In connection with the phenomena M. Dubois discusses the evolution of the sun and the various influences affecting radiation of heat, maintaining that the general evidence of higher and more uniform temperature over the earth's surface prior to middle Tertiary times is well established, and is not interfered with by evidences of extensive though restricted glaciation.

In drawing attention to the influence of winds upon climate during the Pleistocene epoch (*Quart. Journ. Geol. Soc.*, August 1901), Mr. F. W. Harmer has opened up inquiries of considerable and far-reaching interest. Remarking that seasons abnormally warm or cold, rainy or dry, may be caused by the prevalence of particular winds, though the course of the oceanic circulation remain the same, he justly remarks that permanent alterations would equally result were the direction of the prevalent winds permanently changed.

Having attentively studied the causes and influence of areas of high and low pressure, he concludes that the climate of the northern hemisphere could not have been wholly cold during any part of the Pleistocene epoch, and that consequently the period of maximum glaciation in North America could not have coincided with that which affected the British Isles.

Regions covered by ice would have been to a greater or less extent anticyclonic at all seasons, low pressure systems prevailing elsewhere. The northerly winds on one side, either of a cyclonic or an anticyclonic centre, are the necessary equivalent of the southerly winds on the other, the direction in the northern hemisphere in the case of the anticyclone being like that of the hands of a watch, and in the case of a cyclone in the opposite direction.

Thus the effect of the anticyclone of an ice-sheet extending eastward from Greenland, over Great Britain, Scandinavia, and Northern Europe, would have been to change the prevalent alignment of the low-pressure system of the North Atlantic, producing warm south-easterly winds in Labrador and New England during the winter, instead of the northerly winds now prevalent there. The alteration in the direction of the winds would have tended, moreover, to divert the warm surface-currents of the North Atlantic from the European to the American coast.

It is admitted by Mr. Harmer that the maximum glaciation of Great Britain could only have taken place at a time when the Icelando-British channel was closed, either by an elevation of the submarine ridge connecting those countries or by its being blocked with ice. Thus, although the winds have naturally a most powerful influence, which he has done good service in pointing out, he is led to consider that to differential earth-movements of elevation and subsidence in different parts of the northern hemisphere may have been due the suggested shifting of glacial conditions from one side of the Atlantic to the other, and the alternation of glacial and interglacial periods in the eastern and western continents. In this way the milder periods which locally prevailed at intervals during the Pleistocene epoch would be attributed to meteorological and geographical rather than to astronomical causes.

ANIMAL PHOTOGRAPHY.[1]

THE advantages of photography as compared with wood-engraving for the illustration of works on natural history are in many ways so great that any attempt to perfect and popularise the methods in use should be heartily welcomed. Quite apart from artistic effect, the great superiority of photography is that it ensures absolute accuracy, and, when living animals are the subjects, shows them in natural attitudes. In wood-engraving there are several sources of error which only too frequently make themselves apparent. In the first place, the draughtsman may make a blunder. But too often it is the engraver who is in fault, very frequently from mistaking the nature of some feature in the drawing he has to reproduce. For example, the author of the volume before us calls attention to a curious engraver's error in a well-known popular work, where, from some misconception, the mouth of a stickleback appears in a totally wrong position.

Such errors are, of course, impossible in photographs and photogravures. Nevertheless, photography has certain disabilities of its own in regard to animal portraiture. A trained zoological draughtsman, whose object should be to produce a *characteristic* rather than an *artistic* picture, always takes care to draw his subject in a position which will show to the best advantage its distinctive features, whether of form or colour, and for this purpose he generally consults the specialist for whom the sketch is undertaken. The photographer, on the other hand, is usually content to "snap" the animal he has in hand in any effective pose, with the too frequent result that his picture, from a zoological point of view, has comparatively little value. That is to say, the features by which alone the affinities of the animal can be decided are either not shown at all, or are but imperfectly displayed.

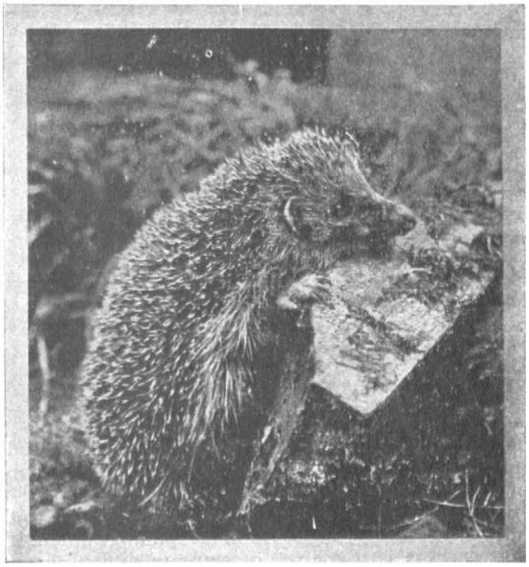

FIG. 1.—Hedgehog.

One of the main objects of the present work appears to be to instruct photographers how to avoid these effects

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[1] "Les Causes probables du Phénomène paléoglaciaire permo-carboniférien dans les basses latitudes." *Archives Teyler*, vii., Partie 4. (Haarlem, 1901.)

[1] "Photography for Naturalists." By D. English. Pp. 132. Illustrated. (London: Iliffe, 1901.) Price 5s. net.

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